Chernoff, Amoz 1993

Dr. Amoz Chernoff Oral History 1993

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Dr. Amoz Chernoff, who was formerly Director of the Division of Blood Diseases and Resources in the National Heart, Lung, and Blood Institute, discusses the implications for the blood supply of the advent of AIDS. He describes the meetings convened at which representatives of the NIH, the CDC, the National Foundation for Hemophiliacs, and the blood banking community met to consider the problems, the development of the grants process to investigate transmission of the disease, surrogate tests, and interagency cooperation. He also discusses his career, the distribution of hematology at NIH, issues in transfusion medicine, and interaction with other countries on hemophilia and transfusion problems.

This is an oral history interview with Dr. Amoz Chernoff at his home in Potomac, Maryland, on 28 January 1993. The interviewers are Dr. Victoria A. Harden, Director of the NIH Historical Office, and Mr. Dennis Rodrigues, Program Analyst, NIH Historical Office.

Harden: We usually start our interviews by asking if you would give us a summary of your background, that is, your education, your medical training, the reasons why you wanted to go into medicine, and your career up till 1980.

Chernoff: That is a tall order. Well, suffice it to say that I graduated from medical school back in 1947 and did a clinical internship and residency at the Massachusetts General Hospital in Boston and at Barnes Hospital in St. Louis, before deciding to go into hematology as a speciality. I had several years of fellowship training in hematology followed by a faculty appointment at the Washington University School of Medicine. Subsequently, in the mid-1950s, I went to Duke University where I became an associate professor of medicine and Chief of the hematology division at the Durham, North Carolina, Veterans' hospital. I had always been interested in hematology. During my medical school years I began to develop an interest, in part, because of the influence of my professor in anatomy who was a man named Tom [Dr. Thomas] Dougherty. He got me involved in bone marrow and bone marrow related activities, and from that my interest in hematology developed. After a few years at Duke University, a new research operation was opening up under the aegis of the University of Tennessee in Knoxville, Tennessee, and I went there as a research professor. A few years later I became Director of the Research Center at the University of Tennessee in Knoxville. The center focused on hematologic problems, on the broad scope of hematology.

Harden: Can I ask you to expand further on your research with hemoglobin and thalassemia.

Chernoff: Starting in my fellowship years at the Michael Reese Hospital, I became interested in hereditary hemolytic anemias and the role that abnormal hemoglobins played in their evolution. In fact, I was one of the people who developed a simple test for fetal hemoglobin, a test which for many years, and perhaps even today, is still used as one of the earliest screening tests for this particular pigment. In any event I became interested not only in the broad group of sickle cell diseases but also other hereditary hemolytic anemias which were subsequently found to be related to the abnormal hemoglobin question. In that group [of abnormalities] are the thalassemias, to which you referred, and that, among other things, led us to study thalassemia in South East Asia and to the discovery of a new hemoglobin called hemoglobin E., particularly among Thais. My colleagues and I went to Thailand in the summer of 1954 to study the disease. We began a rather extended effort among the Thais to study the impact of thalassemia in South East Asia. Some thirty years later in 1984, or 1985, our work was honored by a special award from the King of Thailand and we went back to participate in celebratory activities.

Over the years I focused my research primarily on hereditary hemolytic anemias, specifically hemoglobinopathy, but I also had an interest in other aspects of hematology. I did not, in fact, have a significant interest in transfusion related activities until I came to the NIH. There my focus, while it was in the field of hematology as Director of the Division of Blood Diseases and Resources [DBDR], did shift to areas more related to transfusion medicine. But, to return to the historical aspect in which you are interested, when I reached the University of Tennessee, in 1958 I believe it was, initially I worked primarily in a laboratory. As I have indicated, I became Director of the Research Center, either in 1964 or 1965. I stayed on for some 13 years as Director. Since I have the philosophy that no leader of an organization ought to stay in the same job for more than ten years, I finally prevailed upon my superiors at the university to find a replacement for me. I then assumed the position of the Vice Chancellor for Academic Affairs at the Medical Units of the University of Tennessee in Knoxville, Tennessee.

It was at that time that Bob [Dr. Robert] Levy approached me about becoming Director of the Division of Blood Diseases and Resources at the NIH. Reluctantly I made the move to Washington, D.C., I believe it was in 1978. My title will indicate that I took over the direction of the division from Will [Dr. William] Selzer, who was my predecessor, and I began to focus on the specific aspects of the program that had been developed. My hope in coming to Washington after a very satisfactory career at the University of Tennessee was to try to have some impact primarily on medical education as it relates to transfusion medicine. I was very interested in trying to get a more solid basis for hematologic research, both in the country as a whole and among the students who were going through the learning process at the various universities. I was a little disappointed in realizing how massive the bureaucracy at the NIH was and also how much inertia the system had, so that no matter what one did it resulted in minimal changes of direction.

Harden: Now, are you talking about the NIH or more generally?

Chernoff: The NIH specifically, but more the NIH generally, and the Division of Blood Diseases and Blood Resources more specifically. I was also troubled by what I thought was an unnatural and unworkable division of the field of hematology among two major institutes, actually among four different institutes, of the NIH. It made it hard to coordinate many of the things that could have been done more easily with a more cohesive group of people working together. One of the objectives that I hoped to have some influence on was to try to unify hematology at the NIH and perhaps end up with an institute, a National Institute of Blood Diseases or a National Institute of Hematology.

Harden: This, of course, is a recurring problem at the NIH. The overlap between the National Institutes of Neurology and Mental Health is another example that comes to mind. But I am trying to recall which four institutes were involved in hematology.

Chernoff: At that time Arthritis and Metabolic Diseases had a big chunk of hematology, much smaller than NHLBI [National Heart, Lung, and Blood Institute], but still covering a significant area with some overlap of what we were doing in our institute. Allergy and Infectious Diseases [National Institute of Allergy and Infectious Diseases] had a large part of the immunology that was generally part of hematology and then, of course, the Cancer Institute [National Cancer Institute] had all the leukemias, which again are classically a part of hematology. Among those four institutes, while NHLBI probably covered 60 percent of the ground, Arthritis and Metabolic Diseases, or whatever it is today, covered perhaps 15 to 16 percent, Allergy initially probably covered 10 percent--of course, with the AIDS problem it covered more and more--and the Cancer Institutes probably another 20 percent. This probably adds up to more than 100 percent, but those are the general dimensions. NHLBI had by far the most extensive program in hematology. It was the institute which covered most of the ground, but the others played significant parts in the effort. So that was one of the goals that I hoped, if not to accomplish, at least to get people thinking about. "Why was it that people throughout the community, both the NIH and the hematology community leadership, in general, were not very anxious to see an amalgamation take place?" I thought it was a mistake not to work towards this goal, but that is the way it was. That brings me back to NHLBI and again, after 10 years of leadership of the division there, I went back to my old philosophy which said it was time to move on and I retired from the NIH.

Harden: We will return to that phase of your career later.

Chernoff: I participated in two other activities during the the period after I retired, or resigned, as Director of the Memorial Research Center at the University of Tennessee. I took an eighteen-months sabbatical and became Medical Director of the Cystic Fibrosis Foundation to try to help this private medically oriented public health foundation and interest group put its medical activities on a firmer scientific basis. By that I mean that the Foundation was an effective collector of money and an influencer of Congress but their orientation was largely social. There was some clinical care but they did very little in the way of encouraging scientific research work. And, in that eighteen-month period, I believe I had a significant influence on turning the direction of the Foundation around. We published, with the support of a contract from the NIH, an extensive study of cystic fibrosis and we also published a five-year or ten-year plan which formed the basis of many of the activities that the Foundiation became involved in in subsequent years. They developed research centers, they expanded their support for scientific training and scientific research, and, just in the past two or three years, that support has paid off with the identification of the genetic defect in cystic fibrosis. While I did not have anything to do with the specifics of what emerged, I was able to contribute significantly to the Foundation's orientation and to how they thought about spending their money.

When I retired from the NIH, I took on a brief advisory consultative role at the American Association of Blood Banks, where I was given the official title of the Director for Scientific Affairs. I stayed there some two, two and a half years. I also helped the Association develop a somewhat more scientific orientation. I developed a five-year plan and set up various think tanks that helped explore different areas of research for the future. That is a bird's eye view of my activities.

Rodrigues: One question about your efforts to try and coordinate hematology at NIH. While you were unable to do this in a formal sense, were there any committees or other types of approaches that you tried to employ to coordinate activities?

Chernoff: We did a lot of talking and I met with groups of the leadership of the American Society of Hematology, for example, on many occasions to try to get them to think about the possibility of a more coordinated group. As I indicated earlier, the leadership of the American Society of Hematology was adamantly opposed to bringing all the hematology work into one institute. First of all, I think they felt that they would fail in their efforts to get a separate institute of hematology or blood diseases; second, there was more loyalty on the part of the leadership of the American Society of Hematology to Arthritis and Metabolic Diseases than there was to NHLBI. They were always more supportive of Arthritis and Metabolic Diseases' efforts in Congress than they were of NHLBI's, even though they got the major part of their support from NHLBI. The Society's philosophy was based on the idea that it is better to have an egg in several different baskets. If something happened, they would be able to turn to another basket. It may be true theoretically, but I think it cost the Society heavily over the years in terms of the amount of support and influence they could have had in general, so that was one of my losing battles with the NIH.

Harden: That could be the subject of another separate interview. We could explore the politics. I would like to discuss now when you first become aware of this new disease that came to be called AIDS. We have been told that there were conversations at hematology meetings about patients with immunological defects. We are talking about the period before 1981 when the first paper was published in MMWR [Morbidity and Mortality Weekly Report]. Do you remember when you first became aware that there was a new disease problem and what was being said about it?

Chernoff: I first became aware of the emerging problem, probably in the summer of 1982, after the publication in MMWR of the first three cases of hemophilia who showed up with this disorder. Some people during the summer of 1982 indicated that they knew about other things that were emerging. Oscar Ratnoff was one, if my memory serves me correctly, who seemed to have had a sense of what was emerging during the summer of 1982, but I do not recall ever hearing it discussed at formal meetings. I heard about, but I was not invited to or involved in, a meeting between National Hemophilia Foundation people and the CDC, and perhaps others, sometime in the early part of the summer of 1982. I have not ever seen any minutes of that meeting. It took place, apparently, and some initial discussions were held at it.

During the summer of 1982 I heard rumors and occasional reports either from people or in group discussions. I cannot recall the specifics, but the reports were probably from people, friends, at the CDC. [Dr.] Bruce Evatt was probably involved. This led me to call together in September of 1982, an off-the-cuff, ad hoc group to try to help us deal with the question of what we believed at that time was factor involvement in hemophilia patients or something special about hemophilia patients which made them susceptible to what appeared to be an emerging condition. I was never able to find any minutes of that meeting, but I do know that those attending urged us to support a small project that the CDC had going with the Hemophilia Foundation on immune changes, I believe it was, in the hemophilia population on receiving a lot of blood. We did provide, by an interagency agreement, a small amount of money to the CDC to carry out that study. I do not recall the amounts precisely but it was not a major thing. It was within our ablility to make these kind of decisions more or less on the spur of the moment with the director.

Harden: But, this was definitely before the NIH interagency committee, Bob [Dr. Robert] Gordon's committee, was established and before that famous meeting in Atlanta and phase III.

Chernoff: Yes, I think it was in October of 1982 that we actually issued the interagency agreement and your chronology contains comments that NHLBI supported a study. I am sure that you have the study somewhere.

Harden: Yes. We have been putting together details from a variety of sources by going through various chronologies and files.

Rodrigues: We want to ask a few questions about the meeting in January 1983 that was called by the CDC. When we went through the records that we could find on the meeting, one of the things that confused us was that there seems to be a list of invitees that does not match the list of men who actually attended. Do you recall who attended from the NIH?

Chernoff: There were at least three of us that went to Atlanta in the morning of 4 January 1983. Bob Gordon, Ken [Dr. Kenneth] Sell, and I rode in a cab, I think it was a cab, maybe it was an NIH car, to the airport. From the airport, I think there was at least one other person with us. I am sure that somebody from Allergy and Infectious Diseases apart from Ken Sell, was there. It may have been Tony [Dr. Anthony] Fauci, but I do not remember who it was. The two that I was personally involved with were Bob Gordon and Ken Sell.

Harden: I remember that the three of you were all official designated representatives of the NIH. We came across some more people on a list at some point and we were not sure if they went to the meeting or not. Do you remember who they were?

Chernoff: My name did not appear on the very first list. It did appear on the list as an attendee at the actual conference.

Harden: Can you talk about the conference itself? There were many different constituencies present. You have the government with all its various subdivisions and you have the blood bankers, of which, if am correct, there are two groups, the voluntary blood banks and the commercial ones.

Chernoff: The latter are not blood banks. They are commercial plasma processors.

Harden: Right. But they have different interests, at least according to a couple of items we have read. At this time you knew that hemophiliacs were coming down with AIDS and I believe there had been maybe one or two reports of suspected, if not proven, transfusion-associated AIDS. What was the concern and how do you recall that meeting?

Chernoff: A number of things had happened in the latter part of 1982. Meetings were mostly ad hoc, but I think there was a meeting in December of 1982 in which the Blood Products Advisory Committee was involved. At this meeting some possible issues of hemophilia-related activities were brought up. But the meeting in January 1983, in Atlanta, was promoted as an urgent effort to bring together people who were involved in different aspects of this problem. The idea was to bring them up to date from an epidemiologic and clinical standpoint but also to explore ways in which the blood banking community could address the problems that appeared to be emerging. There was a tremendous amount of interest by people wanting to attend this meeting.

As I recall, our plane was a little late coming in to Atlanta that morning and by the time we got to the CDC, it was probably ten minutes after the start of the meeting. The place was so jammed that we were sitting practically outside the room on seats way in the back. The room was large and filled with people. I do not know how many were there but I would guess at least two to three hundred people must have been crowded into that room. A series of presentations, talks, was given. There was limited discussion in the morning but, in the afternoon hours, there was a lot of discussion. When the discussion turned to what the [blood banking] community should be doing, there was a sense that we did not know what to do but that we should do something. That was the philosophy and there was no consensus about what was the most appropriate thing to do. There were differences of opinion at that point since we did not know what the disease was we were talking about or how it was spread. It was hard to start making basic changes, not so much in industry, but in a medical approach to dealing with the disease of hemophilia on the basis of the limited amount of information that was available. Some people said that the evidence was just not there to suggest an infectious agent as the cause of the disease. There was more discussion about the possible role of nitrites in transmission, the possible role of protein, excessive protein contact, and a variety of other theories that were prevalent at the time. More focus was on what was different about hemophiliacs that made them more susceptible to something that the rest of us did not react to than on what is there in the system that is abnormal or apergenic in causing this disease.

A man from the CDC presented preliminary data on a variety of surrogate tests at that meeting. His name was Dr. Thomas Spira. Data were presented on a variety of patients who had clinical AIDS and on some who had the ARC AIDS-related complex and also some normals. The data were very preliminary. They showed that, in some cases, some of these tests would have identified, or would have been positive in, let us say, a certain percentage of people who had AIDS and in a somewhat smaller percentage of people who did not have the AIDS problem. Those data were presented as preliminary and from a very specialized group of patients. It was interesting that they handed out the data on sheets, but then they collected the sheets again because they did not want the data to get out of the room. The data were considered to be not verified at that point. So data were presented on the role of surrogate tests, that is the possible use of surrogate tests in identifing people who might have the disease. But there was no recommendation by anyone as to the need to institute tests at this point. I think the consensus of the meeting, or at least the feeling that I got out of, was that there was a need to do a lot of research quickly on these various tests. We needed to see if any of them, or any others that might come up, would be suitable for identifying not people who already had AIDS, because they were not likely to be blood donors, but people who were healthy and who might be carrying some presumptive agent for the AIDS disease.

Harden: You have made a very important point in the sense of trying to recapture people's mindset at that time. Looking back, hindsight says why did everybody not recognize that the disease was caused by an infectious agent. What I mean is that the epidemiology was there, but, in fact, I suspect it was not clear. There are people who are saying that the causal agent has to be a virus. It is blood borne, it looks like it follows the hepatitis B pattern. But, as you said, there were a lot of alternative theories at this time. Could you say how you assessed the percentages of people in the room, in terms of who thought what?

Chernoff: I do not think there was an opportunity to evaluate that within the small group with whom I dealt. They were largely blood bank people--people like Lou [Louis] Aledort and Aaron Kellner and Joe [Joseph] Bove--those are the blood bankers, those are the ones that I talked to whose names come to mind. There was real uncertainly about the implications of what was being presented. For one, there was uncertainty as to whether it had anything to do with the use of concentrate or products of blood in general. The blood bank people felt the data were not there to make a statement, or to take a stand, particularly one which was going to have such a profound effect on blood services and the availability of blood in life threatening situations. So, there were many comments about how little we know about the disease and how uncertain we were about the situation. I think that in the group with which I personally interacted, there was a feeling that we needed to do some carefully designed studies as quickly as possible in order to try to resolve the question. We had to define, first of all, or to identify, if possible, what was causing the problem and, secondly, to interdict the possible role of blood as being the vehicle for whatever was involved.

A number of things emerged from that Atlanta meeting. For example, we had a Blood Diseases and Resources Advisory Committee meeting in January that urged our staff in the division to put something together to study the question of surrogate testing for the AIDS situation. As a result of some discussions, Ed [Dr. Edward] Brandt, in the beginning of January, appointed NHLBI and DBDR as the lead agency and division in terms of research related to the development of surrogate tests in the transfusion area. In part because of what the advisory committee did in January, we had on 15 March, I think of 1983, a very important meeting on the current status of the epidemic and the question of testing for it cosponsored, I believe, with Allergy and Infectious Diseases.

Subsequently we developed very rapidly a number of initiatives, one of which was the RFP [Request for Proposals]. The RFA [Request for Applications], having their RFP, moved through the whole system so that we had it on the street in the middle of June or July after clearing Claude's [Dr. Lenfant's] office and the advisory council, first the advisory committee and the NHLBI council. That whole process, from our being designated the agency responsible for this area in the beginning of March to the actual appearance on the street of this RFA, was no more than a matter of three months, which is very fast at the NIH. We received applications in October and I think awarded them the following February so it took less than a year between the initial emergence of the RFA to the funding of the RFA which, again at the NIH, is pretty fast.

To make a long story short, throughout that period of time, even when giving awards to the successful applicants in February of 1984, before we had the virus identified, there was still uncertainty as to: (a) what the cause of the disease was; (b) what specific role transfusion or blood products were playing in it. Needless to say, there were two things happening: first, there was an increasing feeling of urgency on the part of those of us who had anything to do with the transfusion system that we had to assume the worst and plan on the basis of a transmissible agent being involved. While the majority of the people in the transfusion area certainly did not feel that there was enough evidence for a transmissible agent as the cause of AIDS, through the summer that number began to decrease. By the end of 1983, I think most people in the transfusion area felt that there was some transmissible agent involved. Most of us also felt much earlier that we would have to operate on the basis of assuming that there was such an agent. This was earlier in 1983 but that feeling did not even set in probably before the late summer of 1983. The question was still an open one in February of 1984 when we awarded something like two million dollars worth of grants to study this question. When the virus was discovered a couple of months later, or when the announcement of the discovery was made 6 or 8 weeks later, we went back to our awardees and told them that, with the virus having been identified, they should shift the emphasis in their RFA activities, if they could, into more specific tests to identify the virus. That was the sequence of events at least from my standpoint and my recollection.

Rodrigues: Within the Heart Institute, specifically in your division, it was not clear to me from what I have read as to what other components of the PHS [Public Health Service] were also involved with blood safety issues. The CDC called this meeting in January 1983 but which component of the CDC had the lead on transfusion concerns.

Chernoff: I do not know the CDC part very well. Bruce Evatt at the CDC was the one who was most involved with hemophiliacs and the National Hemophlilia Foundation activities and was closest to us, but we also dealt with Jim [James] Allen and someone named Fisher.

Harden: What about the FDA? Same question.

Chernoff: I do not know who was responsible at the FDA but there was a lot of interaction with our division and we met jointly in various environments and various venues, as they say today. We had many discussions with the FDA particularly because of our interest in hepatitis and hepatitis testing and the transfusion transmitted virus study which had been ongoing for a number of years and in which not only the NIH was involved but also the FDA. There is no question that we had a lot of interaction but we had no common responsibilities. We invited the FDA people to our discussions and our meetings and they often invited us to theirs. I think they invited us to a meeting in December of 1982, the Blood Products Advisory Committee meeting in December of 1982. In December of 1983 there was a very important Blood Products Advisory Committee meeting on surrogate testing of which we were cosponsors. Dennis Donahue and I gave the introductory remarks at that meeting, and while we did not have an active role, we were certainly partly involved in what was going on.

Harden: As it was December 1983, it was before the virus was found. What was the major point of discussion? Was it the hepatitis B core antigen that was being recommended?

Chernoff: About ten different people made presentations. Joanna Pindyke presented studies for the New York Blood Center and Herb [Herbert] Perkins presented studies from the Irwin Memorial laboratory on the West Coast. Ron [Ronald] Gilcher talked about studies in the Midwest. The same scientist who talked at the January CDC meeting presented data at the December 1983 Blood Products Advisory Committee meeting on potential surrogate tests totally different than the ones he talked about in January. In other words, in January he talked about core antibodies, about immune complexes and certain hepatitis studies that they were doing, and maybe ALT, things like that. In December he suggested some studies on a urinary protein called neopterin which he found even more specific for picking out people who were AIDS, ARC type individuals. Many different suggestions were made at the December BPAC meeting. Again there was no consensus. I thought it was very interesting that Herb Perkins presented data at that meeting on his so-called zip code study. He had done some of these surrogate tests in areas, zip code areas of San Francisco, which had very low numbers of homosexual people versus other areas with high numbers. He found that the core antibody test, for example, was higher in areas with low numbers of homosexuals, or just as high. Maybe it was not statistically higher, but certainly it was just as high as the rates of positivity of core antibody in the zip codes with high numbers of homosexual people. There was still great uncertainty at that point.

Harden: What was your own feeling?

Chernoff: My own feeling was that we had to assume that there was a transmissible agent involved, but I did not think any of the tests that were being proposed at that point gave sufficient discrimination between those who carry what we later knew was the virus compared to those that did not.

Harden: This is a hypothetical question, but what would you have said to a family member or close friend who called at that time and said, "I need to have surgery. What should I do about the blood supply?"

Chernoff: I do not know what I would have said. Probably I would have said, "If you can get away without using blood in surgery, don't use it. If you were in San Francisco or Los Angeles, or Miami, or New York, which were the four cities with the highest number of cases, I would be very hesitant about taking blood but, beyond that if you don't need it, if you can get away without it, get a surgeon who will deal with you without using blood." It is a little hard to be sure what I would have said because I know a lot more now than I did then.

Rodrigues: In one of the articles that I read on this general subject, the author suggested that throughout the sixties and seventies the way clinicians prescribed the use of blood was incautious, that it had become almost a convenience. The author later suggested that AIDS may have actually had some beneficial side effects in terms of getting people to be more discriminating, particularly in the use of whole blood. What would be your perspective on that kind of observation about the trends in the use of blood and the emphasis on other techniques or other ways of avoiding the use of blood?

Chernoff: I mentioned earlier that I was not really a blood banker. I was not basically focused on transfusion medicine until I got to the NIH and there I became very rapidly involved in all aspects of transfusion medicine. I came in 1978. I would say by late 1979 or the beginning of 1980, I probably felt that blood was being overused; that most single unit transfusions were probably not necessary. If patients could get along with just one unit of blood, the majority of them probably did not need any. I also felt education of physicians in use of blood was something that ought to be instituted. If you look at our various annual plans, in the transfusion area we stress the need to educate physicians in the proper use of blood and blood products. As time went on, we developed this program for specialized centers. There were not scores of them in transfusion medicine, that came later, but there were educational programs. The transfusion medicine academic award was started in the early 1980s. One of the main aspects of it was to educate physicians and medical students in the proper use of blood and blood products. Education was something on which I thought we needed to focus and to encourage in relationship to the general field of transfusion medicine. Incidentally at that time we also started an effort to change the blood resources part of the NIH to the transfusion medicine part. Again if you look back in some of the articles that were written and in the various plans developed in 1982 (I think it was a five-year or ten-year plan that was started during Bob Levy's term), we devoted a lot of space to the question of educating physicians in a more appropriate use of blood but that takes the discussion in another direction. I have forgotten what your original question was.

Rodrigues: My question had to do with the impact that AIDS had on the whole field of transfusion medicine in terms of promoting salvage techniques and alternatives.

Chernoff: We had already begun to think, before AIDS came on the scene, that better education and better use of blood and blood products was necessary. The use of blood was probably increasing much more rapidly than it needed to be. When AIDS came on the scene, particularly by 1984, it was obvious that there were other reasons to limit the use of blood. We recognized that hepatitis was an important consideration in transfusion long before AIDS. When one began to look at the data and found that approximately 3 percent of transfusions were transmitting hepatitis and that the average patient who got blood, received approximately 3 to 4 units of blood during a hospital stay, his or her chance of getting hepatitis from a transfusion as a result of a hospital stay was about 7 percent, which is a very sizable amount. Long before the HIV virus appeared, we were already concerned about the use of blood in questionable circumstances because of its transmission of other diseases. And that feeling, I think, became stronger as time went. Certainly the onset of the HIV virus made it extremely important to modify and moderate the way that we used blood in the clinical setting.

Harden: I have been struck me by how long it took before the full picture of AIDS emerged, that indeed if you got the virus, it might be ten years later before you got the disease. It is 1987 and 1988 before even these facts are shown definitely.

Chernoff: There were three studies we kept in mind. In the mid 1970s, NHLBI and DBDR supported the TTV study. This was the transfusion transmitted virus study which established these 3 percent level I referred to. It established the fact that hepatitis was a lot more common than people had anticipated or knew before that and that the study of ALT levels was one way of being able to deal with possible infectious units. That study went on into the late 1970s and early 1980s. It established the so called non-A, non-B hepatitis, as a significant part of the transfusion transmitted hepatitis problem. It identified the seriousness in terms of numbers of what was happening as a result of transfusion.

When AIDS came into the picture in late 1982, 1983, we began to do similar studies focussing on the AIDS problem. The first thing we did was the RFA to study surrogate tests, which, you remember was our mandated responsibility according to [Dr. Edward] Brandt's directive in early March, which went through [Dr. James] Wyngaarden and others and finally the institute. A second thing that we started was the so-called transfusion safety study (TSS), which had as its goal the study of the effect of multiple transfusions in a variety of diseases. That was the initial goal. Transfusion itself was implicated as being a potentially unsafe procedure. People who had many transfusions had their immune systems affected whether they had viruses or not. The transfusion safety study was aimed at looking at some of these aspects. It focused its efforts on places of both high and low AIDS incidence and involved people who got transfusions for thalassemia, sickle cell disease, heart disease and so forth. Part of that study, a separate letter contract actually, was to collect blood specimens from two hundred thousand donors (before the test for HIV, which did not come onto the scene till 1985), probably from October of 1984 till January or February of 1985, to put those specimens into a repository and to test them afterwards, when the test for HIV became available, and after the blood had been used, and to follow the recipients of that blood.

To summarize, there are three things. The TTV study established the mechanism for doing such studies. It identified non-A, non-B hepatitis as a major problem. It was a study that Will Selzer was very much involved in initiating and I picked up the tail end of it. Then there was the RFA to study surrogate tests specifically and the transfusion safety study which was to look at the role of transfusion in a general sense with the repository being collected to take advantage of the studies that were ongoing in transfusion recipients. Those are three distinct but yet important studies in this whole process.

Harden: It is very good to have that clarified.

Rodrigues: Within the Heart Institute, would you say that most of the AIDS activities were centered in your division or were there other components of NHLBI that were working on the disease?

Chernoff: Initially, it was hard to sell the administration at NHLBI on the importance of this effort. It was not accepted out of hand that it was something that we had to do. We did manage to beat on Claude's [Lenfant's] office hard enough so that eventually he went along with us but he did not appreciate, I think, the magnitude of the problem. There was some reluctance, as I remember it, on the part of Claude and his staff to push the things that we were trying to push. But when Brandt's directive came out, I think Claude recognized both the medical significance and the political value of having a prominent role in this area and so he became much more receptive to the idea. But we had a hard time pushing these major 25 million dollar contract proposals through his office in the beginning.

We were the only ones in NHLBI who did anything related to AIDS for the first few years. All the creative juices, if you will, were on my staff. We initiated efforts to hold meetings, we got people together. For almost eight months, we tried to develop an ad hoc advisory committee dealing with AIDS and transfusion. We were unsuccessful, not because Claude was against it but because Allergy and Infectious Diseases did not want to have such a group. The CDC was against it because they said they already had such a group. But, eventually in January of 1984, we had our AIDS working group put together. June Osborn was the first chairman of that group and she subsequently became the chairman of the AIDS commission. We got her started in this AIDS field.

Harden: Was that within NHLBI?

Chernoff: That was within NHLBI. Initially, I do not think that within NHLBI there was a keen appreciation of the magnitude of the problem.

Harden: There was not any intramural NHLBI research on AIDS that you know of?

Chernoff: Well, there was Harvey Alter but he was not NHLBI.

Harden: He was at the Clinical Center, I believe.

Chernoff: Yes. We supported Clinical Center work. We supported Alter's studies on the chimpanzees again through interagency agreements. We were the only ones supporting transfusion-related AIDS activities at the NIH. We supported Harvey Alter, we supported a chimpanzee colony to provide the animals for him. We helped the CDC through interagency agreements with the transfusion-related activities and we ended up developing this ad hoc advisory committee, on which we had representatives of the CDC, the FDA, Allergy and Infectious Diseases, and a variety of other governmentally related agencies. We had a representative of the National Hemophilia Foundation on it. We had someone from the American Society of Hematology but, basically, it was for our use and it was an internal DBDR activity. After several years, or after a year of meetings, it became an NHLBI committee and then the Lung Institute started to get into it, but the first two years or so we were the only ones. DBDR was the only one that had any formal activity in the AIDS area.

Harden: Was there any effort to support further research on the development of blood substitutes?

Chernoff: That had been going on for a long time.

Harden: Did AIDS have any impact on that work?

Chernoff: It did not have a direct impact because there was already a program on blood substitutes in place from the late 1970s, even before I got to the NIH, and it continued. We supported the peripheral chemical studies for a long time and it may continue even today. They have not really succeeded at it, but there was a new urgency about it. Nothing specific was changed other than to encourage investigators to submit new proposals. We had trouble getting investigators out in the community at large to focus on transfusion or related problems since they tended to fare so poorly at the study section level.

The Hematology Study Section was not attuned to transfusion related activities. The transfusion people felt that they were not getting a decent or fair evaluation from a group of study section members who were more attuned to the cutting edge of genetic activities and immunology, basic science. Transfusion problems were more practical kinds of things and yet there were not enough people who were submitting applications. It is kind of a vicious cycle. If you have a group that is sympathetic, you get more applications. If you have a tough group that is not really oriented to transfusion medicine, then you tend to discourage people from applying. Then the problem is you do not have enough applications to justify a study section. That has been a problem at the NIH for a long time for the transfusion medicine community. During my ten years at NHLBI, in the beginning about 80 percent of the studies supported by the blood resources division were under contracts. By the time I left probably only 40 percent were supported by contracts and the rest were in the investigator initiated effort. But there was always difficulty in investigators getting support at the NIH, not necessarily at NHLBI, because of the presumed reluctance on the part of study sections to give high priorities to studies which were less basic in their orientation.

Harden: Very interesting.

Rodrigues: Were there any ties with any other countries in terms of transfusion medicine at this point? At any of these meetings, were there representatives from other countries?

Chernoff: There were representatives from other countries. I was the NIH representative on the Council of Europe expert committee on blood transfusion during my years at the NIH. To that meeting I presented the first, second, and probably the third, reports on AIDS and transfusion medicine. So I was the original conduit of information about what was going on at the NIH and what I knew about in terms of the hemophilia problem and transfusion problems in general at the Council of Europe. This committee of the Council of Europe was like the FDA in this country. It was an FDA-like structure of the twenty-one member nations of the Council of Europe.

I lectured in Scotland and Germany and I have forgotten where else about transfusion medicine and the AIDS situation. Probably, I was the first one to bring a personal transfer of information, for example, to the Soviet Union, which denied that it had any cases at all. First of all it was illegal to be a homosexual in the Soviet Union, even though you can see homosexuals walking hand in hand in Red Square. But, in any event, I think I was probably the first person to give formal lectures about what was going on in the transfusion community at these various international meetings.

Subsequent to that we always tried to involve foreign countries and have representatives from them over here. The Soviets came several times to this country and spent time with Aaron Kellner and with the blood bank at the NIH. They also went elsewhere in this country. We even encouraged foreign applicants through that RFA, and there was one applicant in Spain that we wanted to support as part of the contract but there was some international difficulties in doing so. I think we did not get as much money as we wanted. Those were the days when you still had to argue about getting a few more million dollars to support a contract. Yes, there was interaction with foreign countries.

Harden: Now, we have the major outlines. What else should we discuss?

Chernoff: There were numerous meetings during 1983, almost weekly or some more than weekly. There were telephone conferences. There were special transfusion reports. I remember we sat in offices at the FDA, and people from the CDC, our institute, and the FDA and others reported week by week what was happening, what new transfusion related cases were being discovered. So there were a lot of meetings. Jim [Dr. James] Wyngaarden set up an NIHwide meeting, and I was NHLBI's representative on that AIDS working group. I told you about our advisory committee--the ad hoc group June Osborn chaired that started in January 1984. We held several formal meetings during the course of 1983--one in September that was focused on epidemiology and surrogate testing; the BPAC meeting in December. We were probably in meetings at least once, if not two or three times, a week during that period; most of them were not formal meetings and no minutes were kept but there was a lot of effort to interact. I was also involved in a number of other meetings for which the Hemophilia Foundation was the primary initiator. They had a meeting in January in New York which I had attended but of which I have very little recollection because it was so crowded. I was sitting practically outside the room and I never did get involved in it. I am listed as an attendee but I do not remember exactly what went on. ABRA, the American Blood Resources Association, which is the overall group of the plasma people, had several meetings in which we were involved. There were meetings all over the place. There was a lot of activity.

Harden: That was in 1983 and 1984 and then, once there was a virus agreed upon, did meetings become more focused?

Chernoff: You became much more directed in what you were doing.

Rodrigues: I saw a few references in the files we were going through that mentioned the NHLBI AIDS working group. But other than the information that the group existed, I have never found anything else about it. I do not know how long it existed.

Chernoff: I have a file of my stuff that I have been able to put together. I do not know if you would be interested in it.

Harden: We would be very interested in it and we can copy it.

Chernoff: The other group I should mention is the Interagency Technical Committee. Dave [Dr. David] Robertson and I were co-chairman of the working group on blood resources. In any event, the first mention of AIDS comes in this meeting, 28 September 1982, in this environment and it was only very brief. This was the first discussion in a formal setting of which there are minutes that I have been able to find. This was in December of 1982.

Rodrigues: We have had, I guess you might say, spotty results in getting records. Some offices are very good in keeping their files and other offices tend to purge things very quickly

Chernoff: My secretary threw out all of my files. I could kill her.

Rodrigues: I know when I went to try and get some of Dr. Krause's records practically everything that they had from the late 1970s and the early 1980s was just gone.

Chernoff: I have the minutes of that 4 January meeting in Atlanta. As you see, Tony Fauci did go to the meeting and I remember Jim Goedert being there. I do not remember Sherman but Ken Sell I remember. Sell, Gordon, and I appeared on the actual attendees' [list] and I think others were there as well. I was just saying that the first mention of AIDS at that NHLBI thing appeared in this 28 September IATC meeting and it is just a brief mention in a paragraph. This was the only summary of the January 4 meeting that I have been able to find. I have the first thing that Claude [Lenfant] ever wrote about our activities. It talks about the conference that we wanted to hold in March, and that we would be a part of another conference in April. Also I have Jim Wyngaarden's authorization, giving us responsibility for this subject.

Rodrigues: That is interesting because I am almost certain that this did not appear in the Office of the Director files or if it did, it is now gone.

Harden: Wasn't there quite a scandal at some point about local blood being contaminated and why the Red Cross was not staying on top of the problem? A second question is about the scandal in France after which people went to prison because of problems in the blood supply. Do you have any comments on what happened and how it all managed to go wrong in both circumstances?

Chernoff: I do not know the specifics of the Red Cross problem that you just mentioned. I have opinions about the French situation but they are based more on what I read in the newspaper and what I recall was the situation at that time. In the French situation, I think the individuals being prosecuted, who have been convicted at this point, are taking the fall for a government decision to promote French development and French industry. It was a conscious decision on the part of the French ruling parties at that time not to let developments from outside the country interfere with internal French developments. I think the people who are convicted, without knowing anything more about it than what I read in the newspaper, are taking the fall for a uniquely French position. This is seen not only in this kind of problem but is frequently experienced in international relations of the French. By that I mean they have a very strong sense of France. French things are primary and you must do everything to protect the French position. The fact that somebody gets hurt in the process is not part of the equation. But, as I say, I think the French people who were involved and convicted are taking the fall for a public policy position that the government is mainly responsible for.

Harden: Were you aware of the French blood test for AIDS that was competing for patent rights with the blood test developed from Dr. [Robert] Gallo's laboratory. You know the brouhaha surrounding that, but were you aware of any differences in the quality, the sensitivity, for example, of the blood test that the French were proposing.

Chernoff: I have no direct knowledge. I understand that the French test was not nearly as specific as the American test in that the quality control on it was nowhere near equivalent.

Harden: There was not a lot of discussion about it that you recall.

Chernoff: There may have been in some circles but not in ours. Another group that we dealt with was Larry Meekie at the Office of Technology Assessments and he was part of our group activities as well. He presented at the September meeting in 1983 of an IATC working group. There was a jointly sponsored meeting on the epidemiology of AIDS, on 13 September, as a part of the Blood Products Advisory Committee meeting. The meeting summarized the different presentations. [Thomas] Spira, whom I mentioned eariler, talked about this stuff neopterin, which he was very excited about at that point. You know that Spira's presentation at 4 January meeting on the surrogate test was never published and, subsequently, I learned that it was turned down by the journals that he submitted it to because the data was not felt to be sufficiently precise, but in August of 1983 they had a lot of laboratory results.

Harden: Well, I believe the effort to talk with people involved with the NIH response to AIDS is worthwhile, because otherwise a lot of material would be lost. We are most appreciative and we thank you for talking to us today.

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